Non-Traditional R & D

Lloyd Fell and David Russell

Uncertainty about **funding**; difficulty in determining **research priorities**; and concern about **technology transfer** (the lack of application of research results): these words stand out in the language of scientific/industrial research and development, today. So-called technology transfer seems to be the central issue because the criteria for determining research priorities and funding decisions are mostly based on the expected "pay off", *i.e.* the economic benefits which will result from the research findings **being put into use** within the industry. This applies, not only in situations where the industry is providing a proportion of the funding, as in most agricultural research, but to scientific research generally which is intended for the "public good."

Why is the practice of science (research and development, rather than teaching), which is intended for our industrial and community progress, subject to these concerns and what is being done to address them?

One response has been to treat technology transfer as a problem requiring research, but this utilises the very same scientific methodology which appears to be letting us down. The demarcation which exists between the physical sciences and the social sciences, or between research and extension (in agriculture), also complicates the situation.

The contextual philosophy of Gadamer and Heidegger and the biology of cognition portrayed by Maturana and Varela could be of assistance in addressing these issues. We claim that it is necessary to examine the fundamental nature of the research and development (or extension) process in order to make a coherent explanation of the

kinds of action which are occurring and to propose alternative kinds of action.

To do this we will examine the traditional operation of scientific research and development (R&D), then draw on the language of contextual philosophy and science to take another look at what scientists actually do, provide an example of a non-traditional R&D project which is in progress and, in conclusion, list what we consider to be the key elements of the conversation which will produce some different kinds of actions and consequences in the context of R&D management.

The Changing Traditions of R&D

Russell and Ison (1991) drew a distinction between **first-order R&D**, in which the researcher remains outside the system being studied, treating it as an objective reality which is independent of the researcher's actions, and **second-order R&D** whereby the researcher and the topic being studied are inextricably linked within the higher-order system. Although the

language of theoretical physics has engendered a certain amount of lip service to the idea of observer-participancy, the practice of science is characterised by the simpler first-order approach and this important epistemological distinction tends to be regarded either as trivial or too messy to address properly.

Thus we have what Winograd and Flores (1987) called a **rationalistic** tradition which has developed through the progress of science since the Enlightenment with the implicit belief that we can eventually reduce everything in the world to definable objects and properties to which certain rules apply. This increasingly knowable world is therefore essentially controllable, the only limitation being the extent of our knowledge. Where we see "problems" in our experience in the world we can **analyse** the situation and find "solutions" by a rational process. We can even take into account how these solutions affect the quality of our lives by adding a social science to the rational process of discovery. It is more traditional to analyse our situation than to **design** our future because we do not care to take into account the idea that the way we see the world could determine what we do in the world, and what we do could determine how we see the world.

The contrast between analysis and design has also been pursued by de Bono (1991). Without the process of analysis most of our achievements in R&D would never have occurred, because we have needed to identify the individual parts and their relationships in great detail in order to gain some measure of control, but there are some difficulties. Analysis applies to closed systems, so we have to assume

that we are dealing with closed systems and make decisions about where to draw the line around the system in question. Our tradition has it that the answers lie in the analysis of data and, for practical purposes, we can ignore the thought that this analysis might not be objective, but might depend on our perceptual framework which tends to consist of rather simplistic models such as correlations, time courses and linear cause and effect.

That the traditions of our R&D have great practical benefit and are workable we would not deny, but we are concerned that there is a crisis of confidence in science arising from what are seen as its undesirable side effects and its failure to "fix" some pressing human problems. Nor would we wish to disparage rationality because we regard our ability to make scientific explanations as, not the only means, but one important means, of designing a better world. What we seek is to enlarge the scope of rational action in science - a commitment similar to that expressed by Winograd and Flores (1987) as a "new foundation for design." Design has to do with what our action generates and how this series of inventions influences our future action. It is only beginning to emerge in our R&D tradition.

We see the first step as recognising the influence of our tradition. Every understanding arises out of a tradition which is a network of prejudices, or pre-understanding, that opens the space of possibilities for that understanding. This tradition is the background against which we interpret and act, largely unaware of the historicity of our thinking. Our main concern, which will be addressed in more detail, is that the insights which arise from our tradition also constitute our blindness, not primarily because our knowledge is incomplete, but because of the very nature of our process of understanding. This has important implications for research methodology and also for the managerial decision-making process which is entailed in influencing the direction of research and the practical application of research results.

The most important and, we believe, pernicious, aspect of our current scientific tradition is its reliance on the information-processing paradigm which has it that the objects and properties of the real world can be represented as bits of information which can be processed and transferred from one person to another as the principal currency of science and most human endeavour. This idea is comparatively recent, dating from about the time of Shannon and Weaver (1949), but it is thoroughly entrenched. It is embroiled in our attempts at understanding two of the biggest issues in scientific R&D today: the use of computers (artificial intelligence, expert systems and decision support systems) and the process of cognition (how do we know about things and communicate this knowledge?).

The emerging intellectual praxis which is known as Social Ecology

(Russell, 1991) espouses the contextual science model which is the counterpoint to information processing, *i.e.* Maturana and Varela's biology of cognition. Social Ecology, which is placed at the cutting edge of changing scientific traditions, deliberately links this with a major tenet of contextual philosophy that practical experience is primary and to be valued above theoretical understanding, being the more fundamental kind of knowledge. Western tradition tends to present theory as clear and experience as cloudy, but we maintain that we do not relate to things primarily through having representations of them; our primary access to the world is in acting without an awareness of the state of our moment-to-moment reflection.

Some Pointers from Philosophy

The hermeneutic philosophy of Gadamer (1975; 1976) and Heidegger (1962), even viewed indirectly through the writing of Winograd and Flores (1987), appeals to us as a significant guide to understanding what is happening with R&D in science. Hermeneutics deals with the interpretation of language, where every reading or hearing is taken to be an act of giving a meaning to the words which is essentially context-dependent. Thus the language we use is that which we have learned to interpret through tradition and we make progress in changing our ways as we change our use of language. Meaning is fundamentally social and based in the action which arises in our language.

The notion of human cognition has been re-orientated in a profound (but difficult) way. The distinction between subject and object no longer applies; the interpreter and what is interpreted do not exist independently; in our *being-in-the-world*, we exist amidst our prejudices which become the normal conditions of experiencing anything. There is never a neutral viewpoint because our assumptions cannot all be made explicit. All that we can do is strive to expand our horizon slightly, or gain a better partial view of our pre-understandings within the social context. Cognition occurs in our praxis or concerned action in the world.

The crux of this for our purpose is the idea that action is not the same as reflection - we are always somehow in the situation. This means we cannot see clearly the everydayness we live in and objects and properties will only arise out of what is called a **breaking down** - a "hiccup" in proceedings, which brings the entities involved clearly into view. This is the space which is available for concerned action; it is not something which has been defined by an objective observer, nor by any individual, as is implicit in the way we do our R&D. The way in which technology arises in the world and the effects which it has are seen in a different light if this philosophical position is entertained.

A Grounding in Biological Science

The Chilean biologist, Humberto Maturana, has been the greatest inspiration to our enthusiasm-for-action about this. This probably reflects our faith in practical science as a solid grounding for working with the difficult circularities which arise in reasoning about second-order R&D. Maturana has pointed out that there is an important difference between doing philosophy and doing science. In the former, it is the philosophical principle which is the issue and is being preserved whereas, in science, it is the observed phenomenon which has to be respected and principles are expendable if they do not do justice to the phenomenon as observed, however that can be determined.

This requires an agreed scientific method which is sufficiently rigorous that another scientist may be able to repeat an experiment and have the opportunity to draw a similar (or different) conclusion. This works well and is very useful, of course, but it is also likely to be mistaken for the objectivity of an independent, external, reality - as in first-order R&D. It is in biology that the evidence has arisen which convinced us that a belief in this kind of objectivity (and the related notion of information transfer) was not serving us well because it obscured our explanations and could be obstructing progress in R&D.

Maturana and others working in neurobiology realised that there was no progress being made by trying to map an apparent external reality of objects and properties onto the nervous system of a living organism - the representationist model did not seem to work. They proposed that the essential organisation of living entailed the systems notion of operational closure (a closed loop) and a self-generating, cognitive, process which Maturana called *autopoiesis* (see Maturana and Varela, 1973; 1980; 1987). The biochemical and physical structure of an organism operates as a network of production which is capable of conserving the identity (or organisation) of the organism as a whole; when this fails, it dies. The structural dynamics, or molecular operations, are the sole determinants of the state of the organism, but at the same time the organisation as a whole has its own properties, which represents a kind of autonomy not previously regarded in biology (see Varela, 1979).

Although it is open to material and energetic exchange with its surrounding medium, the organism is closed to any instructive interaction, *i.e.* information or meaning. What are known as environmental stimuli can only trigger responses non-specifically; the responses are determined by the physiological coherence or structure. Stimuli and responses are not inputs and outputs as suggested in the cybernetic model; physiology is strictly a set of correlations rather than a messaging system and there is no possibility of referring to the

outside from the inside. Thus the information processing idea is a category error in systems logic, confusing a system-external with a system-internal view and obscuring rather than clarifying the biological explanation.

The relationship between organisms and with their environment is a particular kind of structural coupling in which changes within the organism and changes in its surrounding medium are interlocked; they trigger and select one another from the available possibilities, maintaining a structural congruence as long as the relationship exists. Thus we see coordinations of action which we describe as learning, *etc,* without appreciating that this is an observer's view which describes a particular domain of interaction, not the constitutive biological mechanism. We claim that an adequate and complete explanation of the way in which living organisms change (in order to remain themselves!) is this recursive, coupled, triggering interaction between structure-determined (but plastic) entities.

This manifests itself in our networks of conversation. As with the philosophy of Heidegger, the difficulty in seeing this lies in its obvious everydayness. We do not realise that, being only observers, living in actions which can only be described in our language, we bring forth our particular reality. We are not saying that we *create* this reality, but that we bring into operation ("relevate" to use David Bohm's term) its objects and properties by the process of making distinctions in our conversation. Thus the "problems" which we research and the "solutions" which we "discover" do not have the grounds in objective reality which we attribute to them, but they are grounded in our biological process of cognition. This notion of cognition accords closely with the Gadamerian philosophy previously mentioned.

Failing to acknowledge this, we tend not to take responsibility for our actions, attributing them to a situation (and an ethic) which exists outside of us. Our association with Maturana and our colleagues has brought forth our view that everything we say contributes to making our world together and it is a laborious (but potentially rewarding) "bootstrap" sort of process. The two-way effect of our internal state (or emotions) on our possible range of actions and our conversations on our possible internal states means that nothing in conversation is trivial, in a biological sense. Von Foerster (1984) has described this process as "synthetically deterministic," but "analytically indeterminable." Thus we cannot know the future, nor predict with a high degree of accuracy the outcome of research, but we can know that we are contributing to it in a certain way, *i.e.* develop an ethic which is biologically based.

Another Look at What Scientists Do

Traditionally, we refer to science as reductionist in nature, but Maturana has suggested that, strictly speaking, it is not. It is our ability in science to propose generative mechanisms, or operational links, which explain (rather than describe) the relationship between parts which appear separate, that is more important than simply describing smaller and smaller parts. Accordingly, he and Varela articulated four operations as the criteria of validation for a scientific (as distinct from a non-scientific) explanation. We have found it useful to depict this four-step process, which is not dependent on quantification for its integrity, in the following manner:

(1) *describing* a phenomenon that has been experienced and doing this in a way that allows others to agree or disagree as to its existence;

(2) *proposing* an explanation for the existence of this described phenomenon. This explanation functions as a generative mechanism in the sense that, when the mechanism operates, the phenomenon appears;

(3) *deducing* from the first experience, other experiences that are coherent with the first and which would be expected to result from the operation of this mechanism that has been proposed as an explanation; and finally,

(4) *experiencing* the other phenomena that were deduced in step (3).

Although quantification is not essential to this process, it is often useful, of course, particularly in step (3).

Using these operations in science, we begin and end with an experience. We explain experience with experience and the generated explanation remains secondary to the world of daily living. What we may refer to as our new knowledge is only understood in terms of effective action. The particular scientific method used does not give the theoretical explanation any universal validity, but its merit lies in being itself an explanation of a mechanism which can be repeated whenever necessary and referred to as science. Feyerabend (1988) has argued from his interpretation of the history of science that there can be no such thing as a "proper" scientific method (*i.e.* for producing "facts"). In practice, what happens is that our being-in-the-world goes on, but our way of doing something has been changed, not arbitrarily, but through positively addressing a communally-recognised need.

An Example of Non-Traditional R&D

One of us (David) is a Principal Investigator of a current R&D project

in the context of the lives of pastoralists and their families engaged in Merino wool production in the semi- arid region of NSW, to the north of Broken Hill. It is a complex project, which has a team of investigators, each with a different background, engaged in a web of conversation with the pastoralists, because it carries a commitment to second-order R&D. Its progress to date can be checked against the four steps described earlier which are the criteria chosen to validate whether what is being generated is a scientific explanation or not.

The description of the phenomenon (Step 1), or the subject of this study, was the everyday observation that people (including pastoralists) want to take certain actions and not others. They need no persuasion to do what they want to do, but can resist the most sincere attempts to motivate them to do something else (even something which may seem to others to be highly desirable). This phenomenon is particularly evident in the low adoption rates of new technologies or ideas by farmers generally and the concern expressed about this by those responsible for agricultural extension (see Russell et al, 1989; Hartley, 1991). In other words, the research question here is: why do these pastoralists not adopt more of the new technology which is, in theory, available to them?

The explanation proposed (Step 2) as a generative mechanism which might constitutively account for this phenomenon was as follows: *the individual's emotional state of enthusiasm determines the category and scope of actions which can occur*. A corollary of this is that pastoralists in this situation have an enthusiasm-for-action which predisposes to certain types of management practice, but does not permit certain other kinds of action to occur. This explanation derives from Maturana's statement that emotions are, essentially, bodily predispositions for action.

In the course of conversation, some of the pastoralist's enthusiasms-for-action have become apparent. From the operation of these enthusiasms, what other experiences and actions could be deduced (Step 3) which would be coherent with their actions in relation to R&D technology and also be constituted by their particular emotional state? One such indicator was deemed to be: taking ownership of intermediary tasks for facilitating farm management discussions or access to the world of R&D, *e.g.* action-oriented meetings. At this stage, there are indications that this is happening, but further work is needed on the final stage of verification by experience (Step 4).

The role of the researchers includes their part in co-revealing the sequences of actions which affect the adoption of new science or technology by these pastoralists. Accordingly, the conversation is not a "fact-finding" mission, but a sharing of experiences or relating a sequence of events. Thus it includes the narrative type of explanation in which the coherence of happenings over time and the intuitive flow

of meaning is more important than the precision of the data. Agronomic and other data are also collected, with the accuracy required to avoid confusion, but it is via the telling of stories by both researchers and pastoralists that the vital phenomenological data (of experiences) and hermeneutic data (of interpretation) can be recorded and collated into patterns.

Possible outcomes from this sort of research are not entirely predictable. As well as being "analytically indeterminable", they are a communal creation based on personal responsibility, not a discrete technological "fix" applied to a physical problem which existed "out there." The practical value of this work lies in its ownership by the pastoralists themselves, its potential to open doors in new directions, its immediate applicability and its testability as a coherent scientific endeavour.

The phenomenon of blindness to everyday cognitive function (when compared with the external technological fix), can make this type of research appear to some to be rather simplistic or even superfluous. Indeed it seems to us that the more successful agricultural R&D personnel (particularly extension officers) utilise this kind of process while officially operating in the first-order R&D sense. We are referring to what these people are actually doing and to the limitations of working within an outmoded and inappropriate paradigm of technology and information transfer. It is not good enough in science to simply guess at the mechanism, however. Unravelling it carefully leads to stepwise forward progress, *e.g.* the next step could be to address the history of interactions which produced the particular enthusiasms of the pastoralists, today.

Management Associated with R&D

Management is often equated with decision making, which is described as a process of choosing between alternative courses of action - an heuristic search in a given space of possibilities - but this description does not fit the observed phenomenon very well. Winograd and Flores (1987) have shown how this idea (in theory) fails to account for the twin effects (in practice) of the background and being-in-the-situation. It happens that the hard part is formulating the question, or seeing how the alternatives relevant to a particular context came into being. Most problems requiring decisions tend to be fuzzy issues which are not really clear to anyone involved with them, so they are based on a personal judgement and, as we have seen, they are construed in our language, *e.g.* the "energy crisis."

This means that the space of solutions is generated by the commitment in language of those who talk about it; it is not really a matter of choosing, but of generating. By regarding language, not as a

means of transmitting information, but as a "mutually-orienting social action" (Winograd and Flores, 1987), we see that the conversation generates the commitment to action. We design our future in our language. The business of ranking alternatives and choosing between alternatives, which we tend to regard as the most important stuff of management, is far from being the complete story.

The process of determining research priorities, for example, is a particular way of making distinctions which serves the conversation at a relatively high level in an administrative hierarchy where broad resource allocation is controlled. When we acknowledge that these particular distinctions also serve to consolidate our blindness, we appreciate the need to use them at other levels, not as absolutes, but only as triggers to promote a conversation, which is the necessary predisposition to action.

The allocation of funds for research on the basis of (supposedly) clearly-defined, relatively long-term outcomes, while it is a necessary device at a certain level of managerial conversation, is also a construction on which we cannot rely too heavily at the level of action. What appears to us to be really happening in the most effective research is that there is (1) a genuine commitment in language, *i.e.* a desire to follow a particular line of research (a personal enthusiasm-for-action) coupled with (2) a public concern that this could, possibly, be a way of seeing something to which we were previously blind. Therefore research funding is (and should be) influenced more by effective conversation-for-action, than by the distinctions made in setting priorities and goals. It is when someone proposes a new distinction which was not seen in the research program planning that the most effective action occurs. The increasing tendency in some organisations to tie research funding strictly to industry-defined goals, if it narrows the conversation-for-action, may gradually stifle research progress in those organisations.

We are suggesting that an awareness of the possibilities and the limitations of our natural process of cognition, based on modern biology and contextual philosophy, enlarges the scope of managerial options in R&D. It is in positive, uninhibited, conversation that we can make visible a portion of our previous blind spots. The "lateral thinking" of de Bono makes use of this. Expert Systems and Decision Support Systems based on the information-processing model have a role, but it may be a limited role, unless the phenomenon of blindness, which is an integral part of our cognition, can be addressed. Winograd and Flores (1987) discussed the ways in which computers could be more effectively utilised as "tools for conversation" within organisations which are seen as networks of commitments and where the role of management is essentially to take care of these networks.

A Conversation for Change in the Practice of Science

Based on what we have said, we claim that change will occur within a tradition, gradually, by means of actions arising in a network of conversation. We have attempted to list, below, what we think might be some principal elements of that conversation so far as scientific R&D is concerned. We cannot predict the form of the conversation nor the precise nature of the change, but we believe that these elements foster the development of second-order R&D. Crystal ball gazing, either on a grand scale by the world's best experts or in local politics, has been notoriously unsuccessful. Current practice in science and philosophy indicates to us that, acting in a concerned manner, we can see where we are going on a short-term basis and thus we can live according to our particular biological ethics at all times. Ironically, the protection of our future seems to depend not so much on knowing all the long-term consequences as on acting responsibly and rationally today.

The elements are:

1. 1. An **invitation** to join in a conversation in which the other's "story" is respected as legitimate at all times and it is acknowledged that the conversation itself is important. This conversation will include farmers and their families, advisers and researchers and their managers, as equal participants, though with varied talents and skills.

2. 2. A **sharing** of concerns, unresolved questions about what to do next, loose threads or dead-ends in our stories and also hopes and dreams. This is a kind of dialogue through acknowledging *different* ways of seeing things rather than a striving for consensus. In it there is a space created for talking about such matters as the vicissitudes of the farming environment, the underlying ambitions of the people concerned (farmers, advisers, researchers and managers), the "silly" ideas which could not be justified in prudent research or farming, and the gems of wisdom contained in stories from far and wide - an opportunity for listening as well as spelling out.

3. 3. An **acknowledgment** of both the need for managerial distinctions about priorities and goals, in research and on the farm, and the inherent limitation of adhering to these distinctions - because of the phenomenon of blindness in the cognitive process which can turn a creative spiral into a vicious circle. It is in acknowledging that we-don't-know-because-we-can't-see-that-we-don't-know that the space is created for a genuine commitment in language to arise.

4. 4. A **commitment** in language to the resolution of some of the communally-generated issues, or matters of concern - within the network of conversation itself. This involves taking responsibility for characterising the current state of irresolution, *e.g.* costs exceeding the value of production, apparently irreversible land degradation, losses due to pests and disease, *etc.* and designing a stepwise progression towards its resolution, which will have been reached when there is no longer any need for that discussion.

Second-order R&D entails a personal responsibility based on acknowledging the process of cognition, so that the emphasis is shifted somewhat from research priorities and goals *per se* to the nature of the conversation itself. The term "technology (or information) transfer", still serves as an heuristic device (and trigger) to talk about one aspect of this, despite its limitations as an explanation of the mechanism involved. The cognitive biology and contextual philosophy described here appears to us to offer a better explanation and acknowledgment of what it is that the most successful people actually are doing within our present R&D system.

In no sense do we advocate a weakening of scientific practice. By enlarging the grounds of rationality to enable second-order R&D to become a larger part of our tradition, we claim that the practice of science will become a still more useful aspect of our human endeavour.

References

de Bono E. (1991) I Am Right, You Are Wrong. Penguin Books, London.

Feyerabend P. (1988) Against Method. Verso, London.

Gadamer H-G. (1975) Truth and Method (translated and edited by G. Barden and J. Cumming). Seabury Press, New York.

Gadamer H-G. (1976) Philosophical Hermeneutics (translated by D.E. Linge). University of California Press, Berkeley.

Hartley R. (1991) Did you get the message? Agricultural Science, **4** : 40.

Heidegger M. (1962) Being and Time (translated by J. Macquarrie and E. Robinson). Harper and Row, New York.

Maturana H.R. and Varela F.J. (1973) Autopoiesis: the organisation of the living. In Maturana H.R. and Varela F.J. (1980) Autopoiesis and Cognition - the realisation of the living. Reidel, Dordrecht.

Maturana H.R. and Varela F.J. (1980) Autopoiesis and Cognition - the

realisation of the living. Reidel, Dordrecht.

Maturana H.R. and Varela F.J. (1987) The Tree of Knowledge - the biological roots of human understanding. Shambala, Boston.

Russell D.B., Ison R.L., Gamble D.R. and Williams R.K. (1989) A critical review of rural extension theory and practice (a report for the Australian Wool Corporation). University of Western Sydney, Hawkesbury, Richmond.

Russell D.B. (1991) Social ecology as innovative education and research: its actions and its explanations. Studies in Continuing Education, Special Issue - Educating for Change (in press).

Russell D.B. and Ison R.L. (1991) The research-development relationship in rangelands: an opportunity for contextual science. Proc. Fourth Internat. Rangelands Congr., Montpellier, France (in press)

Shannon C. and Weaver W. (1949) The Mathematical Theory of Communication. University of Illinois Press, Urbana.

Varela F.J. (1979) Principles of Biological Autonomy. North Holland, New York.

von Foerster H. (1984) Principles of self-organisation - in a socio-managerial context. In Ulrich H. and Probst G.J.B. Self Organisation and Management of Social Systems. Springer-Verlag, Berlin.

Winograd T. and Flores F. (1987) Understanding Computers and Cognition - A new foundation for design. Addison-Wesley, New York.